

sion—another, to establish one's own. As we have already remarked, the abandonment of Hell's solution is not the demonstration of a satellite; and we have yet to hear the opposite side. Some adverse points we have noted as we have passed along; and we might have added the fact that at the epoch of Rödkier's second observation Uranus and Venus were not far apart; perhaps "within blundering distance." But of course the main strength of the denial lies in the fact that, though the alleged appearance can require but little optical advantages, it has been so frequently sought in vain through a long series of years. During that very spring of 1764, when the primary occupied an especially favourable position, it was very carefully looked for by many observers—among others, the acute and experienced Messier, but nowhere seen except at Copenhagen and Auxerre. Cassini and Short, with interest awakened by their own apparent success, could never with all their diligence recover it; and the latter, twenty-three years after his own striking observation, was thought by Lalande, then in London, to disbelieve the satellite's existence. Not to mention Bianchini and others, the elder Herschel never saw a trace of it; nor Schröter, the close observer of Venus during fifteen years; nor Harding, nor Struve, nor Lamont, Smyth, De Vico, Secchi, or any other of the first observers armed with the first telescopes of modern times. And though the subject has now ceased to attract attention, yet, in the unprecedented multiplication of observers and instruments, it would hardly have had a chance of escape. On the whole, therefore, though the evidence may exclude the intrusion of an ordinary "ghost," it seems irresistible against the reality of a satellite.

What, then, was that which was seen? for that something really has been seen, the character of some at least of the witnesses renders a certainty. A reflection in the telescope independent of the position of the eye would have been always visible as a permanent defect; and the fact of its never recurring is equally adverse to the idea of a satellite, and that of an instrumental deception. The only alternative which remains would seem to be that of atmospheric reflection, or "mirage." There would certainly be some difficulty in finding a parallel among recorded facts, though Brewster, if I recollect aright, speaks of having once seen two images of the crescent moon; but the known instances of atmospheric illusion are some of them so very strange and inexplicable, and yet so abundantly attested, that we may possibly, though with little confidence, seek in this direction a solution of the ancient mystery.

Before concluding these remarks, I may be permitted to relate something which fell under my own notice many years ago, and which may perhaps have some connection with the present subject. The observation which I am about to describe took place in the year 1823; it was not reduced to writing till nine years afterwards, but the recollection of it was then very vivid and fully to be trusted; and a small diagram of the relative position of the objects made at the time in the margin of a pocket-book of that year fixes the date to May 22. Until that evening I had never seen the planet Mercury, but finding that he was then in a favourable position I looked out for him with a little common hand-telescope (my near sightedness and the want of an eye-glass preventing me from detecting him otherwise), and soon found him low in the sunset horizon. The telescope in question had a good achromatic object-glass of 1 3/4 inch aperture and 14 inches focus, and was fitted with a terrestrial eye-piece, magnifying perhaps thirteen or fourteen times; it was a favourite instrument in those early days, and I had succeeded in detecting with it several of the brighter nebulæ and clusters, especially, at the extreme limit of visibility, the large nebula in Triangulum (M. 33). When I had looked at Mercury, I turned to Venus, then high in the S.W., and saw a star, exactly resembling Mercury, or a minia-

ture Venus, ϕ or $s\phi$ the planet, at a short distance, perhaps 20' or 30', and $\frac{1}{3}$ or $\frac{1}{4}$ of its diameter, or rather its impression on the eye, as of course with so low a power the disc of the planet could not be well made out. I had, when I wrote, a very distinct recollection of its great resemblance to Mercury. My mother, who had an excellent sight, coming into the garden, I showed her Mercury and this appearance with the glass, and she not only saw it readily, but we both believed afterwards that she perceived it without that aid. On the next evening, or more probably on the next but one, I could not find it again. As far as I can ascertain, I had in those early days no knowledge of the suspicion that had been entertained of a satellite: and I did not enter it, as in that case I should have done, in a little note-book of remarkable phenomena that I kept. Through the kindness of Mr. Lynn I have been enabled to ascertain that the star ϵ Geminorum was not far from the planet on that day, only about $30\frac{1}{2}'$ further S., which would agree very fairly in that direction, but lying $6\frac{1}{2}$ m. more to the E. Independently of this discrepancy—a serious one, for I have no doubt of the ϕ or $s\phi$ position of the satellite, not only clearly remembered but shown in the little diagram—it does not seem probable that a star of 3-4 mag. should have been so conspicuous in such an instrument in the twilight. I have no note of the hour, but as Mercury had not sunk into the smoke of the town (Gloucester) in the W. horizon, it must have been comparatively early, and at that time of year the twilight is strong. It may be too hazardous under all the circumstances to include this with the other observations of the pseudo-satellite, but there seems no reason why it should pass into entire oblivion.

T. W. WEBB

THE MISSING LINK BETWEEN THE VERTEBRATES AND INVERTEBRATES¹

THE views which Dr. Dohrn has recently put forth as to the details of the steps by which the vertebrate stock arose out of an ancestry not very much unlike the existing Annelids, are of such interest that, notwithstanding previous reference to the subject, no apology is needed for presenting the readers of NATURE with a condensation of the main argument contained in "The Origin of Vertebrates."

Dr. Dohrn first draws attention to the correspondences between vertebrate and insect embryos, which have been too little regarded in consequence of our designating the nervous side in the one as dorsal, in the other as ventral. Yet the facts that, in both, the nervous system is developed on the convex side of the embryo and acquires a strong convex flexure anteriorly, and that the body-cavity is finally closed up on the side of the body opposite to the nervous system, point to a common origin at a comparatively high level. The surface of the animal which is called ventral is determined by the presence of the mouth on that surface; and if any Vertebrates had a mouth-opening between the brain and the spinal cord on the dorsal surface, that dorsal surface would necessarily become ventral. Since, moreover, the ancestors of the Vertebrata must have had a nervous ring surrounding their gullet, it would appear more reasonable to suppose that the mouth-opening had been changed in the course of development than that the situation of the nervous centres had been altered. We are thus led to look for traces of an old mouth-opening on that surface of the early Vertebrates which corresponded to our dorsal surface, and to seek reasons for regarding our present mouth as a comparatively modern development.

Dr. Dohrn believes that the old mouth passed through the nervous centres between the crura cerebelli, or more

¹ Der Ursprung der Wirbeltiere und das Prinzip des Functionwechsels: Genealogische Skizzen von Anton Dohrn. (Leipzig: Engelmann).

accurately, in the fossa rhomboidea, or fourth ventricle, which is remarkable for being of greater proportionate size early in development, and afterwards undergoing retrogression. At an early stage we only need to conceive a slit to be made in the nerve tube at the bottom of the fossa rhomboidea, in order to furnish a suitable passage into the alimentary canal. His first reason for regarding the vertebrate mouth as a modern structure is that it arises so extraordinarily late in development. The embryonic body is almost completely framed, all the great systems are established, the circulation is in active operation, while as yet there is no mouth. Again, the mouth does not arise in the position in which it permanently remains in the great majority. It undergoes considerable shifting forwards. Only in the Selachians and Ganoids does it retain its primitive situation. Moreover, the study of development is steadily tending to establish the idea that the mouth of Vertebrates is homodynamous with the gill-clefts. It is limited, like them, by a pair of arches, lies just in front of the first pair of gill-clefts, arises simultaneously with them in the embryo, and opens into the alimentary canal. A glance at the ventral surface of a Ray shows the likeness of the mouth to a pair of coalesced gill-clefts. Consequently, it becomes probable that the present mouth-opening once existed and functioned as a gill-cleft; that at a certain period in the ascending development, both the old and the new mouths supplied nourishment, that the latter gained the predominance, and that finally the old mouth became aborted.

The next problem attacked is the origin of the gill-clefts. A very elaborate account is given of the supposed process by which the external gills and segmental organs of Annelids were metamorphosed into the gills and gill-clefts of Vertebrates and the skeletal elements connected with them. The great difficulty which Dr. Dohrn confesses in this matter is the connection of the inner extremities of the segmental organs with the wall of the alimentary canal. But if this be granted it is comparatively easy to understand how the shortening and widening of the segmental organs might give rise to gill-cavities such as those of the Selachians. The process by which Dr. Dohrn conceives that the limbs of Vertebrata might have been developed from two pairs of gills in Annelids is a great evidence of ingenuity, though it is to be expected that it will be viewed rather incredulously.

It follows from the view of the origin of Vertebrates thus expounded that Amphioxus loses much of its interest, for there is no place for Amphioxus among Annelids, nor among the primordial Vertebrates; it lacks almost all that they possess. Yet nothing can be gained by excluding Amphioxus from the Vertebrates; for it is so connected with the Cyclostome fishes that it cannot be placed at any great distance from them; while on the other hand it is so related to Ascidiants, that the latter must be included among the Vertebrata.

Dr. Dohrn then proceeds with a long argument to show that the Cyclostome fishes are degenerate from a higher type of fishes, and that Amphioxus is a result of still further degeneration. He shows how their mode of life necessitates many of the modifications they have undergone; and that the diversities of the details of structure in Cyclostomes are inconsistent with their being viewed as representing stages in upward development. Finally, the larva of Ascidiants is represented as a degenerate fish—a degenerate Cyclostome possibly—which carries to the extreme all the departures of the latter from the fish-type. The most important element in this degeneration results from the fact that Ascidiants, instead of being attached to fishes or to any objects from which they can derive nutrition, are fixed to stones, plants, &c., or to such parts of animals (cephalothorax of crabs, tubes of tubicolous annelids) as do not afford them nourishment. Consequently they have lost the old mouth in the organ of attachment, homologous with that of all Vertebrates, and

have developed a new one, homologous with the nasal passage of *Myxine*. Thus we can explain the astonishing fact that the mouth-opening of the Ascidian-larva has a communication with the fore-wall of the so-called cerebral vesicle. It is the last vestige of the openings in the nasal sacs by which the olfactory nerves entered.

The most patent objection to Dr. Dohrn's view about Amphioxus is that it fails to account for the development of a many-segmented respiratory apparatus as a degeneration from a higher animal with a small number of gill-arches. It would appear far more reasonable to suppose Amphioxus to be a degeneration from a much lower elevation than the Cyclostome type, viz., from some stage where the respiratory apparatus retained the multi-septal character derived from its Annelid forefathers.

The keynote of the author's reasonings is to be found in the principle of Transformation of Function (*Functionswchsel*), on which he lays great stress. He states it as follows:—The transformation of an organ happens through a succession of functions being discharged by one and the same organ. Each function is a resultant of several components, of which one constitutes the chief or primary function, while the others are lower or secondary functions. Diminution of the importance of the chief function with increase of the importance of a secondary function, alters the entire resultant function; the secondary gradually rises to be the chief function, the resultant function becomes different, and the consequence of the whole process is the transformation of the organ. This principle is considered to be a complete answer to the difficulty so strongly insisted on by Mr. Mivart, the incompetency of natural selection to account for the incipient stages of subsequently useful structures. Dr. Dohrn's statement of his principle does not strike us as very different from Mr. Darwin's ("Origin of Species," 5th edition, p. 251), though a little more definitely stated. Mr. Darwin says: "The same organ having performed simultaneously very different functions, and then having been in part or in whole specialised for one function; and two distinct organs having performed at the same time the same function, the one having been perfected whilst aided by the other, must often have largely facilitated transitions." The illustrations given by Dr. Dohrn of the steps by which the anterior extremities of Crustacea became applied to mastication, how the mouth of Vertebrates originated from a pair of gill-clefts, how the respiratory apparatus of Tunicates originated from that of Vertebrates, &c., are, however, exceedingly interesting.

An English translation of Dr. Dohrn's pamphlet could not fail to be serviceable to the large number of students who take an interest in the genealogical problems of morphology.

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MAGNETIC OBSERVATIONS IN CHINA¹

THE first annual report of the magnetic observations at this new observatory has just reached Europe, and it contains results of considerable interest to those engaged in the study of terrestrial magnetism.

The position of Zi-ka-wei is 31° 12' 30" N., and 8h. 5m. 45s. E. of Greenwich, being rather less than four miles to the S.W. of Shang-Hai. The observatory is in possession of an excellent set of instruments for determining the absolute values of the magnetic elements, procured by the kind assistance of the Director of Kew Observatory, and a set of self-recording magnetographs by Adie, verified at Kew, have just been erected in a suitable building. The observer, the Rev. M. Dechevrens, S.J., spent a considerable time at Stonyhurst Observatory previous to his departure for China, in order to make himself thoroughly acquainted with the methods of observation, and with the use of the instruments.

The observations in the report extend from April 1874 to March 1875, and furnish the following data for the epoch Oct. 1, 1874:—

¹ "Observatoire Météorologique et Magnétique de Zi-ka-wei." Chine, Magnétisme Terrestre, 1874-5.